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Unbundling democracy: Political rights and civil liberties

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ABSTRACT

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Much recent political economy and political science literature views democracy in terms of political rights. This view, often referred to as electoral democracy, is particularly pronounced in the empirical literature. We reincorporate the role of civil liberties, which are at the core of modern democracy, in two ways. We identify four fundamental sources of potential differences in the evolution of political rights and civil liberties. We present systematic, robust and varied empirical evidence on the direct impact of two of these potential sources of differences using cross-national panel data and accounting for the modernization hypothesis. We obtain two noteworthy empirical results: civil liberties exhibit greater persistence than political rights in affecting subsequent outcomes; and, our main result, civil liberties are complementary to political rights when affecting subsequent outcomes, while the reverse is not the case. Consequently, one must incorporate civil liberties as a determinant of electoral democracy. More generally, both dimensions must be considered to understand the setbacks recently experienced by many democracies, despite their holding of free and fair elections. *Journal of Comparative Economics* **42** (3) (2014) 552–568. School of Economics, University of New South Wales, Sydney NSW 2052, Australia; Economics Department, University of Maryland, College Park, MD 20742, United States.
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1. Introduction

A substantial fraction of economic and political science studies view democracy in terms of the existence of political rights and, in particular, free and fair elections. This view has a long tradition. In economics it can be traced back to Schumpeter (1942). Its origins in political science are more diverse, ranging from Dahl's (1971) procedural emphasis to Przeworski, Alvarez, Cheibug and Limogi's (2000) empirical one. Samuel Huntington – a prominent political scientist – also subscribes to Schumpeter's view explicitly in his influential work (Huntington, 1991, p. 6). Acemoglu and Robinson (2006) reproduce Schumpeter's view in their third chapter, entitled "What do we know about democracy?": democracy is "... the institutional arrangement for arriving at political decisions in which individuals acquire the power to decide by means of a competitive struggle for the people's vote (p. 48). This view is usually referred to as the electoral view of democracy.

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Of course there are other views of democracy, especially in the political science literature. For instance, recently Coppedge et al. (2011) survey the large literature on democracy and identify six conceptions or "models": electoral, liberal, majoritarian, participatory, deliberative and egalitarian. A useful way of differentiating the electoral model of democracy from the liberal model is by noting the main feature of liberal democracy that differentiates the two models: Namely, civil liberties. For example, a distinguished political scientist, Tilly, describes liberal democracy as "...a regime is democratic to the degree that political relations between the state and its citizens feature broad, equal, protected and mutually binding consultations" (Tilly, 2007, p. 13). Subsequently, he explains that "...roughly speaking, political rights correspond to broad, equal, mutually binding consultations, whereas civil liberties refer especially to protection" (Tilly, 2007, p. 45).

This conceptualization of liberal democracy raises three important issues. First, has the standard electoral view left out an important feature of democracy by not considering civil liberties explicitly? For instance, no empirical contribution in economics or political science has considered whether civil liberties are a determinant of electoral democracy. We do so. Second, do civil liberties and electoral democracy evolve in complementary, opposing, or independent fashion? We consider this issue at both the conceptual and the empirical level. Third, it provides a mechanism for differentiation between liberal democratic regimes and electoral democracies that violate civil liberties. Recent years have seen a spate of governments that fall in the latter category. We highlight the differing policy implications of the liberal and electoral models in such cases.

On the first issue, it is worth highlighting an early empirical contribution to the economics literature on the determinants of democratization. Barro (1999) briefly considers civil liberties as an alternative to political rights in defining democracy, but decides to concentrate exclusively on political rights. The rationale provided is that (p. 177) "...the economic and social forces that promote electoral rights are similar to those that stimulate civil liberties." Neither Barro nor any subsequent contributors to this empirical literature, however, considers whether civil liberties operate as a determinant of electoral democracy.

Similarly, some political scientists have considered civil liberties in general terms. For instance, views of democracy tied solely to the holding of free elections are referred to as minimalist and they are contrasted to an alternative insisting on "...a more ample degree of protection of political and civil liberties" (Plattner, 2002, pp. 56–57). Other political scientists have referred to Schumpeter's view and similar ones in political science, such as Przeworski et al. (2000), as sub-minimal (Mainwaring et al., 2007, Chapter 5). They propose additional properties to eliminate "defective" or "illiberal" democracies from classification as democracies. Nonetheless, this literature has not investigated empirically whether civil liberties are a determinant of electoral democracy.

On the second issue, by viewing political rights and civil liberties as two different but equally fundamental dimensions of democracy we pursue an important question that has not been asked in the literature – Do these two components behave as complementary, substitutable or independent dimensions in the evolution of liberal democracy? In the analysis of consumption, complementarity refers to the case where a rise in price of a good, which decreases the quantity of its use, also leads to a decrease in the quantity of use of another good (substitutability and independence refer to cases where the price rise leads to an increase or no change, respectively). In the case of democracy, if changes in one dimension of democracy induce changes in the same direction for a second dimension over a given timeframe, one can view them as evolutionary complements; if such changes induce opposite ones in the second dimension, one can view them as evolutionary substitutes; and if there is no effect of one dimension on the second, one can view them as evolutionarily independent. Thus, in principle, the existence of two separate dimensions generates a variety of possible patterns in the evolution of democracies.

Finally, on the third issue, glaring inconsistencies arise between supposedly democratic regimes of various vintages in terms of electoral rights and their undemocratic behavior in other aspects. Egypt is a dramatic recent example. While the Egyptian elections in May of 2012 were generally seen as free and fair, sophisticated commentators readily admitted in justifying the recent coup that the country's governance since then has been highly undemocratic (Ignatius, 2013). One of them notes that "[Egyptian President] Morsi governed as a thug..." (Milbank, 2013). This example illustrates a basic problem that is pervasive in affecting many recent democracies in all regions of the world. Namely, societies can manage to have free and fair elections symbolizing the provision of political rights; yet, they can be underperforming substantially in the provision of civil liberties. Examples from around the world abound: in the Middle East and North Africa, Arab Spring countries and even Turkey are affected. Among transition countries, Hungary and Russia provide illustrations of the problem. Among Latin American countries Argentina and Venezuela illustrate different variants of the same basic problem. These illustrations suggest that the more nuanced view represented by liberal democracy might be a better guide to effective policy in promoting democracy.

Our work departs from an important strand of literature on democracy in both the economics and the political science literature which focuses on electoral democracy defined in term of political rights. A prominent example in economics is the seminal work by Acemoglu et al. (2008) [henceforth AJRY]. It has led to a large, increasingly specialized and rapidly growing literature addressing issues raised by their main finding. The finding by AJRY that the level of per capita income has no impact on democracy in the long run once fixed effects and endogeneity are accounted for has attracted considerable attention. It rejects an important—if not the most important—component of Lipset's (1959) long standing modernization hypothesis, which has been also supported more recently by Huntington (1991) and others. In the economics literature, this finding has generated subsequent contributions focused on political rights but relying on the use of more advanced econometric methods and expanded data sets to reverse the conclusion, e.g., Benhabib et al. (2011) and Che et al. (2013). It also led to a subsequent contribution in the same vein by Acemoglu et al. (2009). Most recently, one contribution splits their sample into former colonies and non-colonies to do so (Cervellati et al., 2014).

An equally prominent example in political science focusing exclusively on political rights is the contribution by Przeworski et al. (2000, Chapter 2). It has also led to a growing subsequent literature. One strand relevant in our context

has emphasized the impact of development in terms of per capita income on two different aspects of democracy: its stability, which is one of Przeworski et al. (2000) two main findings, and its lack of effect in bringing democracy into existence or democratization, which is the other one. Boix and Stokes (2003) challenged this second finding. More recently Boix (2011) proposed and found empirical evidence in support of what he calls 'conditional modernization' theory. Namely, his panel data work supports the stability effect, the positive effect of income on democracy over the long-run (meaning going back into the beginning of the 19th century) and its lack of effect or amelioration of this effect in the short-run (meaning after World War II).

With respect to this strand of literature in both economics and political science our work posits and answers the following question – Does it make any difference to the role of income in explaining the evolution of electoral democracy (if any such role exits) that civil liberties are omitted from the explanation? Including civil liberties has no effect on the role of income as a determinant of electoral democracy, reduces the impact of political rights persistence in explaining electoral democracy, and shows that prior civil liberties play a major role in explaining electoral democracy.

Mention should also be made in this context of a somewhat related strand of literature in economics that assesses the impact of short-run aggregate shocks to the rate of economic growth on democracy measured in terms of political rights. For instance these shocks are weather and export revenues, Burke and Leigh (2010), within country variations in rainfall in sub-Saharan Africa, Bruckner and Ciccone (2011), and oil price shocks, Bruckner et al. (2012). Restrictions on the number of countries, methodologies and focus on specific shocks limit their applicability and usefulness in the present context. Thus, we will not pursue these issues here. Nonetheless, the same question on the potential impact of civil liberties on their results also arises.

One issue that we will consider in our sensitivity analysis is the role of other variables that may influence democracy, besides persistence, income and civil liberties, in an empirical setting. For this purpose we rely on the insights derived from earlier results, including Barro (1999) and Acemoglu et al. (2009). In addition we draw on the recent political resource 'curse' literature. While the political science literature on this topic has focused on the impact of oil wealth (or other sources of non-earned income) on the stability of democracy and dictatorship, e.g., Morrison (2007, 2009), we rely on the economics literature focusing on the effect of oil wealth on democratization, e.g. Tsui (2011). The latter finds a negative effect of oil endowments on electoral democracy which he explains by arguing that rents from a resource like oil can be effectively controlled by the state. Thus, political leaders have added incentives to monopolize control of the state and limit political competition in order to protect their access to the oil rents.

Our work also relates to more recent studies assessing whether a broad set of institutions, particularly cultural norms, affect the pattern of democratization across countries. For instance, in a recent working paper, Gorodnichenko and Roland (2013) show that the degree to which a country's culture reflects individualist (rather than collectivist) values affects the changes in its political rights over the past three decades. Our paper offers evidence through which historically inherited cultural values may affect present day political outcomes, assuming these cultural values shape the degree of civil liberties in a polity. Furthermore, our conceptual framework suggests mechanisms for their operation. Indeed, an insightful recent paper by Czeglédi (2013) relies on our differentiation between the degree of appropriability of rents within civil liberties to model such a mechanism.

The plan of the paper is as follows. We indicate carefully what we mean by political rights and civil liberties in the next section and discuss the corresponding empirical measures. More importantly, we identify fundamental sources of differences in the potential evolution of these two dimensions of democracy at the conceptual level, indicate their two main empirical implications and relate these to our baseline empirical specification. Subsequently, in Section 3, we discuss the data underlying our empirical analysis.

Section 4 focuses on analyzing the basic dynamics of unbundled democracy by observing the effects of civil liberties in explaining political rights, and vice versa, in terms of our baseline empirical specification. Using the most widely employed empirical techniques in both economics and political science (least squares), we identify important characteristics of these two dimensions in the evolution of democracy that continue to hold throughout the rest of the paper. First, civil liberties have a substantial impact on the role of persistence in political rights on electoral democracy and no effect on the role of income. Second, while civil liberties are complementary to political rights in their evolution, political rights have no effect on the evolution of civil liberties. In Section 5 we look at the impact on the results of applying GMM to address panel data bias in dynamic settings to the same data set and empirical specifications.

We provide three sets of robustness checks in Section 6. First, we consider data features such as boundary issues in the FH measure, an alternative index of democracy in terms of political rights (the Polity IV index), and extensions of the sample time frame. Second, we consider the issue of balanced versus unbalance panels in the AJRY setting. Third, we consider the impact of the political resource curse and other potential determinants of democracy highlighted in the literature in terms of potential omitted variable bias. In all these settings, our two main results on the role of civil liberties in the evolution of democracy continue to hold. A brief conclusion provides perspective and draws implications.

2. Conceptual framework: fundamental differences between political rights and civil liberties

Political rights are widely accepted as an essential dimension of democracy in recent political economy and political science literature. Their definition commonly revolves around the provision of free and fair elections. Most directly, they

involve providing an electoral process with these characteristics at the executive, legislative and local or regional level (the latter level is often ignored, but it matters and especially in large and/or populous countries). One step removed is the provision of an environment free from intimidation and coercion for open and broad participation by citizens as voters, candidates and members of political parties. Finally, these rights also include the provision of mechanisms that link the policies undertaken to their control by elected leaders in transparent ways that lead to accountability. Freedom House's political rights index is the empirical measure most directly linked to these features.[1] Table A1A reproduces the 3 categories and the 10 questions, scored on a scale of 0–4, used to construct this political rights index. Each question has a 1/10 weight in the index.

While civil liberties are in principle widely recognized as an essential element of democracy in terms of protection of individual rights, they tend to be neglected in practice, as indicated in the introduction. Osiatyński (2009, p. 2) makes a distinction between individual rights, which he characterizes as emerging in the 18th century, and human rights, which he views as a 20th century concept. Individual rights have been recognized as essential characteristics of democracy over the last two hundred years, embedded as they are in many countries' constitutions. These individual rights are often referred to as first generation human rights. They usually include freedom of speech, freedom of assembly and a category that is much more difficult to describe. It is sometimes referred to as due process protection, equal treatment under the law or protection from arbitrary treatment by the state.

The concept of human rights, however, is somewhat broader and Osiatyński (2009, Chapter 1) describes its evolution to include additional ones that are not necessarily individual in nature. A narrower interpretation of additional human rights, however, has been adopted in the economics literature and referred to as "second generation human rights" by Kaufmann (2004) and others. These additional human rights—which are of post-WWII vintage—include, for instance, secure ownership rights and individual mobility (in the pursuit of economic betterment) with respect to location, education and employment. They have been viewed over the last several decades as part of the array of civil liberties to be provided and protected by a democratic government; for example, Freedom House includes them as part of its civil liberties index (see Piano and Puddington, 2006). We include these narrower second generation human rights in our concept of civil liberties as an essential characteristic of democracy and we use the Freedom House measure of civil liberties in our empirical work.[2] Table A1B of the Appendix reproduces the 4 categories and the 15 questions, scored on a scale of 0–4, used to construct this civil liberties index. Each question has a 1/15 weight on the index.

With these clarifications as a preamble, we note several fundamental differences between these two dimensions of democracy relevant for the evolution of democracies. An important potential reason for the differential evolution of these two dimensions of democracy is the following: Citizens' enjoyment of political rights such as the right to vote or volunteer to campaign for someone yields utility only indirectly—that is, through the policies enacted by those for whom a citizen voted or campaigned. Whatever citizens expect to obtain from exercise of their political rights, there are two sources of uncertainty intervening between their actions and a desired outcome. First, their choice of politician or position needs to prevail in an election. Second, the politician or the position needs to implement the citizens' action as intended or promised. One reflection of these additional uncertainties is the substantial literature in public choice and political science explaining why citizens bother to vote in the absence of a clearly defined self-interested motivation to do so, e.g., Feddersen (2004). In contrast, there is no substantial literature explaining lack of demand for freedom of expression, assembly, the desirability of not being imprisoned by the state, or the desire for secure property rights or mobility across location and employment.

An important reason for the absence of the latter literature lies in that citizens' enjoyment of civil liberties such as freedom of speech, assembly and choice of location to live and work usually yields utility directly with far less uncertainty, if any, between the citizens' action and the realization of the desired outcome. It follows that these additional uncertainties in the realization of benefits from exercising political rights are likely to lead most individuals to value them less than they value the exercise of their civil liberties. Direct empirical implications arising from this fundamental difference are testable but likely to require microdata or experiments. Indirect ones, however, arise more easily as they underlie incentives behind the next two potential reasons for differences in the evolution of both political rights and civil liberties. These potential reasons generate testable implications with aggregate data of the type used in prior studies cited in the introduction.

In a representative democracy, the exercise of political rights by voters or politicians often acts as a constraint imposed on politicians or on a small groups of agents in a discrete manner – that is, at particular times and in particular contexts. For instance, this would be the case for electoral supervision by competing parties or for separation of power conflicts between the executive and the legislature resolved by the judiciary. By contrast, the exercise of civil liberties by citizens requires constraints imposed on the state that enhance the activities of all agents in a far less discrete or intermittent manner over time, space and individuals – that is, commitments to refrain from predation by protecting first and second generation human rights presumably apply all of the time and to all citizens, at least in modern times. An important empirical implication of this feature that we will test is the following: civil liberties should exhibit greater persistence than political rights in their

[1] Alternative measures are also used empirically. Among them the most prominent one is the Polity IV index, which we will consider as a robustness check in Section 4.

[2] Just as in the case of political rights, there are other indexes used empirically. In contrast to the case of political rights, however, none of them have the same prominence that the Polity IV index has and, perhaps more importantly, their coverage of civil liberties is far more narrowly focused than the Freedom House measure.

impact on their own subsequent outcomes. Furthermore, the strength of the incentive toward persistence should also be greater for civil liberties than for political rights due to their direct rather than indirect impact on utility.

Parenthetically, most of the literature on democracy and especially the one associated with AJRY stresses the importance of long-run factors in the development of institutions, e.g., Acemoglu et al. (2005). Hence, it has become customary in the empirical literature on democracy to include a lagged term to capture the persistence effect of electoral democracy. Our argument extends this approach to civil liberties and suggests that the impact of persistence on civil liberties should be greater than the impact of persistence on political rights.

There are interactions in the production of these two dimensions of democracy that suggest precedence in time for some civil liberties relative to some political rights in specific settings. For instance, civil liberties like freedoms of association and speech are naturally crucial for the emergence of competitive political parties that take part in free and fair elections. Similarly, second generation human rights may also be important for the production of political rights if, for example, equitable access to education shapes the emergence of representative political leaders. As a result, one can think of some civil liberties as precursors to some political rights. An important empirical implication of this feature that we will also test is the following: one expects to observe empirical relationships where the levels of civil liberties would play a role in determining subsequent levels of political rights but perhaps not vice versa. Once again, the more direct nature of incentives for demanding civil liberties relative to those for demanding political rights reinforces a tendency for asymmetries in their evolution.

An illustration of these asymmetries arises in a historical setting. In an insightful paper on women's rights and economic development, Fernandez (2014) develops a model that shows how economic growth leads men to prefer a system with equal rights for women to one where men enjoy a monopoly over rights. A main implication of the model is that a decline in fertility induces men to reform the property rights system toward equal rights at an earlier date. She tests this implication of the theory in the following manner. First, she identifies two important property rights for women acquired through the legal system, control of their own separate estate and ownership and control of their earnings. Second, she creates a dummy variable that dates when they were both first simultaneously available to women in different states of the contiguous USA. Third, she uses this variable as her dependent variable and finds supporting evidence for her theory relying on data from the early 19th century to the 1920's. In the course of doing so she notes "...in general property rights preceded voting rights: only five states allowed women to vote prior to the reform of property rights."

Both of these empirical implications provide the basis for our baseline empirical specification, which includes the baseline specification in the prior literature as a special case. They can be stated precisely as follows.

$$PR_t = \alpha_1 PR_{t-1} + \beta_1 CL_{t-1} + \lambda_1 Y_{t-1} + \varepsilon_{1t} \tag{1}$$

$$CL_t = \alpha_2 CL_{t-1} + \beta_2 PR_{t-1} + \lambda_2 Y_{t-1} + \varepsilon_{2t} \tag{2}$$

where $\alpha_2 > \alpha_1$, $\beta_1 > \beta_2$, $\lambda_j \geqslant 0$ for $j = 1, 2$ and Y_{t-1} represents per capita income. The first two inequalities capture our two empirical implications. If one sets $\beta_1 = 0$, and ignores Eq. (2), one obtains the main baseline specification from the prior literature. Incidentally, in general the complementarity effects (β_1 and β_2) are not symmetric because the concept of complementarity here corresponds to the notion of gross complementarity. Hence, it is possible for lagged CL to be complementary to PR [$\beta_1 > 0$ in (1)] and for lagged PR to be independent of CL [$\beta_2 = 0$ in (2)], or vice versa.

More generally, there are additional implications for the differential evolution of political rights and civil liberties that we do not seek to test empirically. These are associated with the appropriability of rents generated by political rights and civil liberties. Rents generated by political rights are directly appropriable by politicians. That is, citizens' enjoyment of political rights when acting in their role as politicians generate substantial rents as private goods that are concentrated in space, time and, of course, individuals, e.g., Keane and Merlo (2010) provide monetary estimates of these economic benefits in the US Congress. By contrast citizens' enjoyment of civil liberties generates substantial rents for society from the provision of these civil liberties as public goods. These rents arise in two ways: indirectly through first generation human rights leading to innovations from knowledge creation and transmission, Aghion and Howitt (1998); and directly through second generation human rights leading to increased output from reductions of uncertainty and transaction costs and improved allocation of resources, which allow the operation of modern or 'socially contrived' markets at a high level of transactions, BenYishay and Betancourt (2010).

For both first and second generation human rights, the rents generated through the enjoyment of these civil liberties tend to be more dispersed in space, time and individuals to whom they accrue than the ones generated through political rights. This dispersion makes it more difficult for rent seekers to appropriate these rents, whether they be dictators or democratic politicians, than the ones generated by political rights. Thus, it generates another powerful reason for the differential evolution of political rights and civil liberties. This differential appropriability of rents generated by political rights and civil liberties has been ignored in the literature to our knowledge. Similarly, rents generated by second generation human rights tend to be more dispersed in space, time and individuals to whom they accrue than rents generated by first generation human rights. Czeglédi (2013) relies on this differential appropriability within civil liberties to develop a model in which differential enforcement costs due to attitudinal differences among cultures lead to different equilibria. The latter are characterized by governments providing different levels of civil liberties as a result of differential returns to rent seeking activities.

3. Data sources

As our primary measures of the dimensions of democracy, we use the civil liberties (CL) and political rights (PR) data from Freedom House, which are available at annual intervals between 1973 and 2009[3]. Incidentally, the FH measure is used either as a primary measure of political rights or as a robustness check on any other measure used as the principal measure in most empirical studies of democracy. Standard practice in the empirical literature is to start with the PR and CL variables measured by FH on a 1–7 scale. FH develops these indexes by converting the aggregate scores, ranging from 0 to 40 and 0 to 60, respectively, onto the 1–7 scale. Lower scores represent better conditions. To make our results more easily interpretable, we follow the literature converting these measures onto a [0,1] scale, with higher scores representing better conditions. We also follow the literature in focusing our investigation on effects at 5 year intervals.

On the measurement side, FH democracy indexes have been criticized in political science on two grounds: aggregation of the political rights index and the civil liberties index to construct a single indicator, e.g., Munck and Verkuilen (2002), and having a bias (especially during the Cold War era) due to the assignment of higher scores to regimes politically aligned with the US. The first criticism does not apply to us nor to strands of literature identified in the introduction because neither our work nor the recent literature relies on the aggregate index. Recently, however, the second criticism has been found to apply to the political rights index although it has diminished in the post-Cold War era, Steiner (2012). To ensure this bias does not drive our results, we conduct sensitivity analyses, reported in Section 6, that vary the time intervals. Given the time-varying nature of the bias, such variation in the time span of the analysis would yield differing estimates if this bias is driving the results. We find little change in the estimates across time spans.

We construct our initial sample by focusing on those countries in which the FH PR and CL data and income data are available in the 1970–2000 time period. We begin with the sample of countries for which FH data is available and impute the 1970 CL and PR values using the earliest observation in 1973.[4] We then restrict our data to those country-year observations with income data from the Penn World Tables [PWT] (version 6.3, benchmarked to 2005 PPP dollars). As noted by Benhabib et al. (2011), version 6.3 of the PWT includes many observations that were missing from previous versions (including version 6.1, used by AJRY).[5] Our data thus includes 915 observations in 175 countries over the reference time period.

To understand the dynamic interactions of CL and PR, we focus our analysis on balanced panels for which the full time series of CL and PR are available for the 1970–2000 period. Unbalanced panels can generate consistent estimates with greater precision when the reason for the observations' exclusion is uncorrelated with the disturbance term in the regression of interest. In our case, however, countries that enter the sample mid-period are typically those that are newly independent and are likely to experience quite different dynamics in their PR and CL from previously existing countries. Including these observations is likely to lead to inconsistent estimates. Consequently, a balanced panel of 131 countries with 786 observations on PR and CL over this period becomes our baseline sample throughout the paper. We check the results with the initial unbalanced panel in the sensitivity analysis of Section 6.

Part A of Table 1 reports summary statistics for PR and CL in the baseline sample. These measures incorporate both cross country and within country variations. For instance, the within-country standard deviations of PR and CL (0.183 and 0.142, respectively) are much lower than the ones reported in the table. Similarly, the sample correlation between political rights and civil liberties across countries and over time in this sample is 0.92. But this measure also incorporates both cross-country and within-country correlations. Our fixed effects regressions examine only the latter, which are also lower in magnitude.

When we revisit AJRY's results on income and democracy in Appendix Table A2, for example, we return to our initial sample. One of the instruments used in AJRY is the savings rate. We also make use of the updated PWT data on government and private consumption to calculate the national savings rate, data which are available for 866 observations for 162 countries in our sample.[6] Part B of Table 1 reports summary statistics for the main variables in this initial sample.

In the analysis of other determinants of democracy in Section 6, we make use of several additional data sources. When examining the relationship between oil and democracy, we constructed a separate sample of country observations for which reliable oil reserve data are available. The data on oil reserves come from Dr. Colin Campbell at the Association for the Study of Peak Oil (ASPO), a non-profit organization gathering industrial data to study the dates and impact of the peak and decline of world oil. These data are a particularly useful source because they include oil discoveries and thus permit credible computation of real changes in oil reserves. The total oil reserves in this dataset are measured as the cumulative quantity of oil discoveries minus the cumulative quantity of oil production as of year *t*. Thus, changes in reserves in a given period reflect

[3] Available online at http://www.freedomhouse.org.

[4] AJRY also further supplement this data with data from Bollen (1990, 2001) for political rights in 1950, 1955, 1960, and 1965, obtaining 945 observations for these countries. Because comparable data are not available for CL for these early years, we restrict our sample to the years 1970–2000. When we replicate AJRY's estimation using this subsample, we find qualitatively similar results for the effects of GDP per capita on political rights. These results are reported in column 2 of Appendix Table A2.

[5] These new country-years observations are spread over 40 countries, and are quite different from those that AJRY use in their estimation: The levels of political and civil freedoms in these countries are much lower than those in the AJRY sample, and while their mean levels of income are comparable to those in the AJRY sample, their changes in income over this time period are significantly lower. When we replicate AJRY's estimation adding these new observations from version 6.3, we also find qualitatively similar results for the effects of GDP per capita on political rights (see column 3 of Appendix Table A2).

[6] AJRY relied on these data to obtain 2SLS estimates. We replicate their 2SLS estimation for PR with both their original sample of countries and our extended sample. Again the results are qualitatively similar, which can be seen in columns 4–6 of Appendix Table A2.

Table 1
Summary statistics.

	Num countries	Num obs	Mean	SD	Min	Max
Panel A: Basic dynamics sample						
CL	131	786	0.499	0.321	0.00	1.00
PR	131	786	0.495	0.373	0.00	1.00
Panel B: Democracy and income sample						
CL	175	915	0.510	0.320	0.00	1.00
PR	175	915	0.514	0.374	0.00	1.00
Ln GDP pc	175	915	8.485	1.153	5.03	11.31
Savings rate $(t-2)$	162	866	14.850	26.331	−243.30	85.74
Panel C: Democracy and oil sample						
CL	77	409	0.493	0.323	0.00	1.00
PR	77	409	0.510	0.382	0.00	1.00
Total oil reserves	77	409	170,839	1,274,760	0.00	12,999,827
Per capita oil	77	409	0.377	1.635	0.00	19.48
Median age	77	409	24.125	6.930	14.40	41.30

the net change in discoveries and production over that period. Cotet and Tsui (2010) describe these advantages of the ASPO data on reserves over other data sources in more detail.

With respect to five oil producing former Soviet countries, we had to impute missing pre-1991 observations. This was accomplished by fitting their post-1991 data on that of several comparator countries (Canada, Great Britain and Romania) and predicting the pre-1991 reserves based on these comparator observations (we verify in a robustness check that these observations do not drive our results). We thus obtain data on oil reserves for 77 countries that have at least one period with positive reserves. Part C of Table 1 provides descriptive statistics for these countries. We impute oil reserves as 0 for all countries not included in the ASPO dataset. Thus, for our analysis of other determinants of democracy we start with the same set of 131 countries and 786 observations in our original balanced sample. Parenthetically in analyses with oil in the data set, we re-scale the oil variable to be measured in 10 trillion barrels for ease of interpreting coefficients.

Our data on educational attainment comes from the Barro and Lee (2010) dataset, which includes age-specific mean years of education at five year intervals between 1970 and 2000 (the complete dataset extends to 1950–2010). To construct the parental generations' educational attainment, we follow Barro and Lee (2010) and calculate the mean years of education for all those aged 40–75 (weighted by each cohort's population share). These data generate a reduced set of 708 observations in a balanced sample of 118 countries.

Finally, we add data on total population counts and the urban share of the population from the World Development Indicators. These data generate 768 observations in a balanced sample of 128 countries for the demographic variables. We also use the Muslim share of the population from the Pew Forum on Religion and Public Life to split the sample, which is explained in Section 6.

4. Basic dynamics of unbundled democracy: least squares results

We now turn to assessing the empirical evidence on the evolution of political rights and civil liberties using our baseline empirical specification of liberal democracy. By examining the dynamic evolution of these two variables in simple terms, which are captured in Table 2 (columns 1–4), we can gain greater insight into these dynamics and their relation to the ones in the baseline specification of electoral democracy. We first introduce persistence effects by themselves, controlling for year effects and country effects, in a balanced panel of 131 countries for the years 1975–2000 and present the results in columns (1) and (2). Our panel relies on 5 year intervals to capture longer term changes while retaining a time series dimension. These simple regressions suggest strong persistence effects for both dimensions of democracy in terms of magnitude and a high level of statistical significance (0.1% or $p = .001$). The estimate in column 1 is within the range of estimates obtained for the baseline specification of AJRY reported in Table A2 of the Statistical Appendix, which also control for year and country fixed effects. The estimate in column 2 is novel and similar in magnitude to the one in column 1. Furthermore, it shows a similar reduction in magnitude due to the introduction of country fixed effects than the one in column 1. This can be seen by comparing them to the estimates in columns 1and 2 of Table A3 of the Statistical Appendix. The latter presents the same results as Table 2 without country fixed effects.

Columns 3 and 4 of Table 2 allow us to examine the impact of introducing complementarity effects on the two initial regressions with fixed effects. The introduction of lagged civil liberties in the political rights equation (column 3) reduces the persistence effect of political rights in column 1 by 58%, while the introduction of lagged political rights (column 4) in the civil liberties equation reduces the persistence effect of civil liberties in column 2 by 15%. Consequently, the civil liberties persistence effect (α_2) is far greater in magnitude than the political rights persistence effect (α_1) as suggested by our first empirical implication. Perhaps more importantly, the CL persistence effect is significantly different from zero at the 0.1% level whereas the PR persistence effect is significantly different from zero only at the 5% level. Similarly, the complementarity effect of civil liberties on political rights (β_1) is positive, as sizable in magnitude as the persistence effects, and statistically

Table 2
Unbundling democracy, baseline specification.

Estimation	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS		OLS		OLS		2SLS	
Dependent variable	PR	CL	PR	CL	PR	CL	PR	CL
PR ($t-1$)	0.348***		0.146*	0.0644	0.144*	0.0595	0.115	0.0509
	(0.058)		(0.071)	(0.046)	(0.071)	(0.045)	(0.079)	(0.047)
CL ($t-1$)		0.377***	0.351***	0.320***	0.350***	0.318***	0.317***	0.290***
		(0.046)	(0.079)	(0.051)	(0.079)	(0.051)	(0.086)	(0.052)
Ln GDP pc ($t-1$)					0.0226	0.0454+	0.305	0.0970
					(0.027)	(0.024)	(0.303)	(0.105)
Observations	786	786	786	786	786	786	757	757
R-squared	0.813	0.848	0.822	0.849	0.822	0.850		
Num countries	131	131	131	131	131	131	131	131

Robust standard error clustered by country in parentheses.
Country and year fixed effects included in all columns.
*** $p < 0.001$.
** $p < 0.01$.
* $p < 0.05$.
+ $p < 0.1$.

different from zero at the 0.1% level. On the other hand, the complementarity effect of political rights on civil liberties (β_2) while positive is small in magnitude, and not statistically different from zero even at the 10% level. This confirms our second empirical implication.

We explore how these results change in our baseline empirical specification that introduces per capita income as an explanatory variable for each dimension of democracy. If Lipset's view of modernization is correct and per capita income is an important determinant of either or both dimensions of democracy, the previous results might suffer from an omitted variable bias if per capita income is correlated with either lagged CL or lagged PR. If the AJRY view is correct and per capita income has no effect on democracy, this variable should not have an effect on these results. Columns 5–6 of Table 2 show that our two fundamental empirical implications continue to hold in this specification. The results on the persistence and complementarity coefficients in both equations differ from the corresponding ones in columns 3 and 4 only in the third decimal. The impact of per capita income on political rights is positive but not significantly different from zero even at the 10% level, which is the same as AJYR's finding. Its impact on civil liberties is also positive and significantly different from zero but only at the 10% level.

It is customary in the political economy literature (but not as much in the political science literature) to follow AJRY and correct for the potential endogeneity of per capita income. Columns 7 and 8 of Table 2 do so by estimating our baseline empirical specification with 2SLS, following AJRY in the use of the two period lagged savings rate as an instrument for per capita income. The persistence effect of civil liberties continues to be positive, substantial in magnitude, and significantly different from zero at the 0.1% level. By contrast the persistence effect of PR, while positive, is small in magnitude and, in contrast to the corresponding estimates in columns 1, 3 and 5 of Table 2, is not significantly different from zero even at the 10% level. Similarly, the complementarity effect of CL on PR is positive, substantial in magnitude and statistically significant at the 0.1% level, whereas the complementarity effect of PR on CL is positive but very small in magnitude and, similarly to the corresponding estimates in columns 4 and 6, not significantly different from zero even at the 10% level. Per capita income now has no effect on either dimension of democracy, even at the 10% level of significance.

Summing up our substantive results, introducing civil liberties in the standard baseline empirical specification of electoral democracy in the literature generates the same results as before for per capita income (namely, no effect), eliminates the persistence effect of political rights at the 1% level of significance, and establishes a strong complementarity effect of civil liberties on political rights at the 0.1% level of significance. Estimating our baseline empirical specification for the civil liberties equation shows no effect of per capita income on civil liberties at the 5% level of significance, no complementarity effect for political rights on CL even at the 10% level of significance and a strong positive persistence effect for civil liberties at the 0.1% level of significance. These results also convincingly confirm the two empirical implications of our conceptual framework for liberal democracy: namely, $\alpha_2 > \alpha_1$ and $\beta_1 > \beta_2$.

In Table A4 of the Statistical Appendix we report a set of robustness checks on these least squares results for our baseline empirical specification. For instance, we estimated the two equations implied by the liberal democracy view as a system relying on 3SLS. The results with and without country fixed effects are reported in columns 1–4. The ones with country fixed effects (columns 3 and 4) are quite similar to the corresponding ones in Table 2 (columns 7 and 8) and confirm the two empirical implications derived from our conceptual framework. The other checks are: use of a shorter balanced panel (1980–2000), dropping observations due to outliers selected on the basis of Kennedy's DFBeta procedure (Kennedy, 2008, Ch. 20); and dropping savings rate outliers selected by eliminating observations outside the 5th and 95th percentile of values for the savings rate. Our two main empirical implications hold without exception in all these settings. We also added an interaction term between lagged civil liberties and lagged political rights in the specification corresponding to columns 3 and 4. The estimated coefficients for the two interaction terms had t-ratios less than unity.

Besides these statistical results, we also considered what outliers in our data set suggested for our substantive implications. When looking at outliers over the sample period, one observes that Panama in 1985, Honduras in 1975 and Ecuador in 1975 have the largest positive differences between civil liberties and political rights (i.e., CL–PR). If we look at the same information in 2000 (the final year in our sample), we find that the difference had disappeared in all three cases (in the case of Panama, PR had actually improved beyond CL). In all three countries, however, political rights had improved substantially and civil liberties had remained the same (Honduras) or improved substantially. On the other hand, we see a different dynamic when looking at the three outliers with the largest negative differences (in absolute value) between CL and PR (i.e., PR–CL) over the sample period (India, Sri Lanka, and Syria). In two cases (India and Sri Lanka), political rights had remained unchanged by 2000; these rights had actually decreased in Syria. Over the same time period, civil liberties experienced no change in one case (Syria), a minor increase in another (Sri Lanka) and a substantial one in the third (India).

Thus, the analysis of outliers suggests it is better for democratic outcomes to start with higher levels of CL than of PR. This suggestion is consistent with the two empirical implications discussed in Section 2 and with the findings of our statistical analysis on the impact of civil liberties on the persistence of political rights and on the persistence of civil liberties. These results are novel as well as important. Are they due to dynamic panel bias?

5. Basic dynamics of unbundled democracy: dynamic panel bias

It is well known that the introduction of fixed effects biases the coefficients of lagged dependent variables towards zero, which is referred to as dynamic panel bias. A systematic way of exploring the role of dynamic panel bias is facilitated by the introduction of some notation. The results presented in Table 2 for the democracy outcome variables, PR and CL, can be rewritten in terms of the baseline empirical specification of columns 7 and 8, as follows:

$$Democ_{ijt} = \alpha_j PR_{ijt-1} + \beta_j CL_{ijt-1} + \lambda_j Y_{ijt-1} + \gamma_{ij} + \delta_{tj} + \varepsilon_{ijt} \tag{3}$$

where $j = 1, 2$ and $Democ_{i1t} = PR_{it}$, $Democ_{i2t} = CL_{it.}$ γ_{ij} is a country-specific fixed effect in the jth equation and δ_{tj} is a year-specific fixed effect.

Using lagged dependent variables as regressors introduces dynamic panel bias because those lags will themselves be correlated with previous observations' error terms (e.g., PR_{it-1} will be correlated with ε_{1it} for $s < t$). While this bias disappears as the number of periods increases (as $T \to \infty$), our sample includes only 5 periods. A starting point to address this issue is the instrumental variables (IV) approach proposed by Anderson and Hsiao (1982). This requires specification of the model in first differences and the use of two-period lags of PR and CL as instruments for the respective first differences. These instruments lead to a consistent estimator through OLS estimation of the following first difference equation for each outcome variable.

$$\Delta PRi_{t,t-1} = \alpha_1 \Delta PR_{it-1,t-2} + \beta_1 \Delta CL_{it-1,t-2} + \lambda_1 \Delta Y_{it,t-1} + \Delta\delta_{1t,t-1} + \Delta\varepsilon_{1it,t-1} \tag{4}$$

$$\Delta CLi_{t,t-1} = \beta_2 \Delta PR_{it-1,t-2} + \alpha_2 \Delta CL_{it-1,t-2} + \lambda_2 \Delta Y_{it,t-1} + \Delta\delta_{2t,t-1} + \Delta\varepsilon_{2it,t-1} \tag{5}$$

While consistent in the absence of second-order autocorrelation in levels, this estimator is inefficient. The Arellano and Bond (1991) difference Generalized Method of Moments estimator (DGMM) improves on the efficiency provided by the Anderson–Hsiao IV estimator by using available lags greater than two periods as instruments in the difference equations. Presence of first order serial correlation in the error terms of the levels equations, however, would also lead to invalid instruments in DGMM, as this correlation makes the two period lagged levels invalid instruments for the one period lagged differences. One solution is to restrict the instrument set to only lags of PR and CL of three or more periods. We adopt this procedure.

Finally, the literature indicates that the DGMM estimator can also suffer from the problem of weak instruments. A proposed solution for this problem is to rely on system generalized method of moments (SGMM), Arellano and Bover (1995), by adding (stacking) the level equations and using first differences as instruments for the levels while still checking for serial correlation to ensure the validity of the instruments. This solution is valid provided the assumption of zero correlation between the deviations of the dynamically evolving variables from their long run means and fixed effects holds, Roodman (2009).

Table 3 presents the results of correcting for dynamic panel bias for PR and CL using the DGMM method and the SGMM method for our baseline empirical specification (columns 3 and 4 and 7 and 8, respectively) and after eliminating the per capita income variable (columns 1 and 2 and 5 and 6, respectively). In all cases we use the three period lags as instruments because we find evidence of second order serial correlation or of invalid instruments when using two period lags as instruments. We also use the two period lagged savings rate as an instrument for per capita income. The first point that emerges from this table is that the DGMM estimates are more unstable than the SGMM estimates. For instance, the introduction of per capita income through the baseline specification changes the coefficient of complementarity (lagged CL in column 1) in the PR equation from a magnitude of 1.053 (column 1) and statistical significance at the 1% level to a magnitude of .766 (column 3) and statistical significance at only the 10% level. Similarly, it changes the persistence coefficient in the CL equation from a magnitude of .505 (column 2) to a magnitude of .144 (column 4). Note that the variable introduced turns out to be statistically insignificant in both column 3 and column 4. Incidentally, the point estimates of the coefficient of per capita income are negative in both columns (3 and 4) which is what AJYR found using DGMM. Hence, we will concentrate on the SGMM results (columns 5–8).

In contrast to the DGMM results, the SGMM results are robust to the inclusion of per capita income in the baseline empirical specification. The coefficient of complementarity in the PR equation (column 5) has a magnitude of 0.933 and is

Table 3
Unbundling democracy, dynamic panel bias?

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Difference GMM (3 + lags as instruments)		Difference GMM (3 + lags as instruments)		System GMM (3 + lags as instruments)		System GMM (3 + lags as instruments)	
	PR	CL	PR	CL	PR	CL	PR	CL
PR ($t-1$)	−0.363	0.0782	−0.252	0.120	0.0697	0.0797	0.0732	0.0932
	(0.361)	(0.294)	(0.340)	(0.247)	(0.254)	(0.195)	(0.249)	(0.194)
CL ($t-1$)	1.053**	0.505	0.766+	0.144	0.933**	0.600*	0.925**	0.604*
	(0.366)	(0.331)	(0.442)	(0.347)	(0.315)	(0.304)	(0.304)	(0.292)
Ln GDP pc ($t-1$)			−0.132	−0.166			0.0130	0.0104
			(0.142)	(0.105)			(0.025)	(0.019)
Observations	524	524	524	524	524	524	524	524
Num countries	131	131	131	131	131	131	131	131
Num instruments	16	16	17	17	23	23	24	24
Lags as instruments for PR/CL	3+	3+	3+	3+	3+	3+	3+	3+
p-Value for ...								
AR(3)	0.146	0.133	0.126	0.233	0.341	0.132	0.346	0.140
Hansen J	0.313	0.0677	0.421	0.338	0.256	0.262	0.236	0.198
Diff-in-Hansen (Lagged differences, null: difference subset is exogenous)					0.821	0.96	0.846	0.920

Robust two-step standard errors estimated with Windmeijer (2005) small-sample corrections in parentheses. Year fixed effects included in all specifications; country fixed effects excluded, following Roodman (2009). Both PR and CL variables are instrumented with their relevant lagged levels and differences in each specification. Per capita income is instrumented with the two period lagged savings rate.

*** $p < 0.001$.
** $p < 0.01$.
* $p < 0.05$.
+ $p < 0.1$.

statistically significant at the 1% level; the introduction of per capita income has hardly any effect on this coefficient (column 7). It takes on a magnitude of 0.925 and remains statistically significant at the same 1% level. Similarly, the persistence coefficient in the CL equation takes on a value of 0.600 without per capita income in the equation (column 6) and it becomes .604 as a result of its introduction (column 8). It is statistically significant in both cases at the 5% level. It is also worth noting that the point estimate for persistence of political rights is negative with the DGMM estimation method but becomes positive with the SGMM estimation method. The same applies to both point estimates of the coefficient of per capita income. They become positive with the SGMM estimation method. Incidentally the pattern of the results with respect to per capita income is similar to what is found in the literature: Namely a negative coefficient with DGMM as in AJRY (2008) and a positive coefficient with SGMM as in Che et al. (2013).

On the technical side, our use of GMM in this context illustrates a couple of points. First, the greater stability of the SGMM estimates is consistent with the fact that for the same number of observations, this estimation uses seven more valid instruments than the corresponding DGMM estimates. This is not surprising in light of Bond's (2002) conclusion when comparing DGMM and SGMM estimates that "...By contrast the extended estimator, system GMM, has much smaller finite sample bias and much greater precision when estimating autoregressive parameters using persistent series." Second, we do not have the problem of too many instruments due to the few lags that arise in our balanced sample once autocorrelation tests are satisfied. Hence, we do not report results using Roodman's (2009) procedure for collapsing instruments. We did run the procedure, however, and the results are similar.

Summing up our substantive results after correcting for dynamic panel bias with SGMM, introducing civil liberties in the standard specification of electoral democracy eliminates the persistence effect of political rights, has no effect on the role of income per capita on political rights and establishes a strong complementary effect of lagged civil liberties on current political rights at the 1% level. Estimation of our baseline specification for the civil liberties equation shows no effect of per capita income or of a complementarity effect for political rights even at the 10% level, and a strong persistence effect for civil liberties at the 5% levels. More generally, the two empirical implications of our conceptual framework are also confirmed after correcting for dynamic panel bias: Namely, $\alpha_2 > \alpha_1$ and $\beta_1 > \beta_2$.

6. Sensitivity analysis

In this section, we consider the sensitivity of our results to several issues. The first section evaluates the extent to which our baseline specification results are affected by the discrete nature of the FH variables, the political rights index used, and the timeframe studied. The second section discusses the extent to which our findings would differ if we were to rely on the unbalanced panels often used in the literature. Finally, we investigate the extent to which our findings would differ by incorporating the main variables used in prior literature as determinants of democracy. We address the extent to which these variables may be acting as possible omitted variables and biasing our results when left out of the estimation.

6.1. Baseline specification revisited

In Table 4, we consider whether the constrained nature of the Freedom House PR and CL measures affects our results. We begin by dropping boundary observations—either at the lower bound of zero or at the upper bound of 1—and repeating our baseline fixed effects specifications (those in columns 7 and 8 of Table 2). We find our results largely unaffected (columns 1–4). Indeed at the 1% level of statistical significance, the 'largely' can be eliminated from the previous sentence.

We find very similar results when we use the Polity IV index instead of the FH PR measure (columns 5–6 of Table 4) as our measure of political rights. That is, the persistence effect is higher for civil liberties (0.323) than for this measure of political rights (0.257), i.e., $\alpha_2 > \alpha_1$, and the confidence intervals do not overlap. Furthermore, the complementarity effect of civil liberties on political rights (0.348) is substantial in magnitude and statistically significant at the 0.1% level whereas the coefficient of the alternative political rights measure (0.0828) has no statistically discernible effect on civil liberties even at the 5% level ($\beta_1 > \beta_2$). This result also provides reassuring indirect evidence that Cold War era bias in the Freedom House political rights index is not driving our findings.

Timeframe changes provide direct evidence on the robustness of our results and also that Cold War bias in the FH indexes is not driving the results. In columns 7–8 of Table A4, we extend the sample timeframe beyond that of AJRY, adding FH data from 2005 and 2010. The additional observations provide greater precision, reducing our standard errors and making the coefficients on PR statistically significant in both regressions at the 5% level. Nonetheless, the complementarity effect of PR on CL remains statistically insignificant at the 1% level and substantially smaller in magnitude than the complementarity effect of CL on PR. In sum, $\alpha_2 > \alpha_1$ and $\beta_1 > \beta_2$ just as before. Finally, in columns 9–12, we vary the intervals in our panel framework from 5-year windows to 3- and 7-year windows. In both cases, our two main empirical implications hold: $\alpha_2 > \alpha_1$ and $\beta_1 > \beta_2$. Not surprisingly, we find stronger persistence effects and complementarity effects in the shorter interval that has more observations.

6.2. Unbalanced panel results: baseline specification

In the first four columns of Table 5, we present the least squares results analogous to the last four columns of Table 2, but relying on the unbalanced panel data set described in Section 3. In this unbalanced sample the substantive results for 2SLS (columns 3 and 4) are essentially the same as in Table 2 (columns 7 and 8) at the 5% level of significance, despite the difference in countries and observations. Similarly, the substantive results for OLS (columns 1 and 2) are essentially the same as in Table 2 (columns 5 and 6) but at the 1% level of statistical significance, since the persistence effect of political rights—previously significant at the 5% level—now disappears.

Summing up, our main substantive results with LS estimation are the same as in the balanced panel. The persistence effect of CL is statistically significant at the 0.1% level, and substantially greater than the persistence effect of PR (which is not statistically different from zero even at the 10% level) with both OLS and 2SLS. Similarly, the complementarity effect of CL on PR exists and is statistically significant at the 0.1% level while the complementarity effect of PR on CL does not exist

Table 4
Sensitivity analysis of LS estimation for baseline specification.

Sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Drop observations @ lower bound of dep var (=0)		Drop observations @ upper bound of dep var (=1)		Full sample		Full sample extended to 1970–2010		3-Year intervals 1970–2000		7-Year intervals 1970–2000	
Dependent variable	PR	CL	PR	CL	Polity IV	CL	PR	CL	PR	CL	PR	CL
PR, $t-1$	0.249*	−0.00243	0.175*	0.0738			0.218***	0.0864*	0.389***	0.0775*	0.0445	0.0651
	(0.123)	(0.040)	(0.077)	(0.049)			(0.058)	(0.038)	(0.048)	(0.037)	(0.092)	(0.069)
CL, $t-1$	0.342**	0.338***	0.250**	0.282***	0.348***	0.323***	0.357***	0.369***	0.321***	0.486***	0.277**	0.130+
	(0.114)	(0.054)	(0.092)	(0.060)	(0.084)	(0.055)	(0.065)	(0.043)	(0.056)	(0.041)	(0.100)	(0.077)
Polity IV, $t-1$					0.257**	0.0828+						
					(0.087)	(0.046)						
Ln GDP pc, $t-1$	0.00253	0.0411	0.00100	0.0414	0.00291	0.0593+	−0.00581	0.0261	0.000907	0.0310*	−0.0177	0.0335
	(0.046)	(0.039)	(0.029)	(0.025)	(0.040)	(0.032)	(0.019)	(0.016)	(0.019)	(0.016)	(0.044)	(0.035)
Country FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Obs.	402	546	510	588	684	707	1,108	1,108	1,534	1,534	591	591
R-squared	0.835	0.823	0.662	0.724	0.845	0.845	0.844	0.871	0.863	0.870	0.821	0.839

Robust standard error clustered by country in parentheses.
*** $p < 0.001$.
** $p < 0.01$.
* $p < 0.05$.
+ $p < 0.1$.

Table 5
Democracy and income, unbalanced panel.

Estimation	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS		2SLS		DGMM (3 + lags as instruments)		SGMM (3 + lags as instruments)	
Dependent variable	PR	CL	PR	CL	PR	CL	PR	CL
PR, $t-1$	0.103	0.0420	0.0896	0.0390	−0.446	0.0402	−0.264	−0.0834
	(0.068)	(0.045)	(0.072)	(0.046)	(0.391)	(0.309)	(0.373)	(0.256)
CL, $t-1$	0.393***	0.332***	0.369***	0.310***	1.230**	0.317	1.279**	0.786*
	(0.076)	(0.051)	(0.080)	(0.052)	(0.448)	(0.410)	(0.426)	(0.326)
Ln GDP pc, $t-1$	0.0181	0.0387	0.191	0.0548	−0.0695	−0.171*	0.00933	0.0185
	(0.029)	(0.024)	(0.247)	(0.110)	(0.101)	(0.072)	(0.020)	(0.019)
Observations	915	915	866	866	575	575	575	575
Num countries	178	178	165	165	152	152	152	152
Num instruments			1	1	17	17	24	24
p-Values for...								
AR(2)							0.937	0.0636
AR(3)					0.183	0.202	0.279	0.0970
Hansen *J*-test			0.0604	0.0936	0.123	0.0326		
Diff-in-Hansen (Lagged diffs)							0.938	0.651

In columns 5–8, robust two-step standard errors estimated with Windmeijer (2005) small-sample corrections in parentheses. Year fixed effects included in all specifications; country fixed effects included in columns 1–4 and excluded in columns 5–8, following Roodman (2009). Two-period lagged savings rate is used as an instrument for GDP per capita in columns 3–8.

*** $p < 0.001$.
** $p < 0.01$.
* $p < 0.05$.
+ $p < 0.1$.

even at the 10% level with both OLS and 2SLS. Finally, in the unbalanced panel per capita income has no effect on either dimension of democracy even at the 10% level of significance.

In the second half of Table 5 (columns 5–8), we present the dynamic panel bias correction results analogous to columns 3 and 4 (DGMM) and 7 and 8 (SGMM) of Table 3 but relying on the unbalanced panel. The DGMM results are similar to the unstable ones we found in Table 3 but in this case the Hansen J-test indicates invalid instruments at the 10%level. Similarly, the SGMM estimates show more variability than before. Moreover, in the CL equation one must accept the hypothesis of second order serial correlation at the 10% level and of invalid instruments at the 5% level for the Hansen *J*-test. Notwithstanding, the SGMM estimates are consistent with the two main empirical implications of the conceptual framework at the 5% level of significance, i.e., $\alpha_2 > \alpha_1$ and $\beta_1 > \beta_2$

While the results of the unbalanced panel with least squares do not seem much affected, the results with either DGMM or SGMM are sufficiently weak and unstable to indicate caution in the use of unbalanced panels when explaining democracy while applying GMM methods. Perhaps an alternative or supplementary explanation to the possibility of bias due to the correlation between the reason for exclusion of observations and the disturbance term in the dependent variable lies on the loss of statistical power in the GMM techniques. The need to rely on three period lags due to second order serial correlation in GMM estimation leads to LS estimates with 915 observations while GMM methods rely on 575 observations.

6.3. Omitted variable bias: other potential determinants of democracy

In this section, we investigate the impact of other potential determinants of democracy on our earlier results. We rely on the prior literature discussed in the introduction for guidance on the choice of variables to consider, but we improve the data or the estimation procedure whenever feasible. Just as in previous sections, we begin with the least squares results. In contrast to those sections, however, we skip the usual OLS results and go directly to the ones that account for a variety of econometric problems associated with these new variables. These results, which also include country and time fixed effects just as before, are presented in Table 6.

Columns 1 and 2 of Table 6 present the results of adding to the 2SLS estimations in Section 4 (columns 7 and 8 of Table 2) the log of population. In order to account for both the potential endogeneity of this variable due to reverse causality with democratization as in Alesina and Spolaore (1997) and/or the effect of population momentum as in Keyfitz (1971), we use population lagged three periods (i.e., 15 years) as an instrument. Our earlier results on the persistence and complementarity effects of civil liberties relative to political rights go through exactly as before, as can be seen through a comparison with the corresponding columns of Table 2. Moreover, population's impact on either dimension of democracy is statistically insignificant at any reasonable level.

Since per capita income has a *t*-ratio of less than unity in both columns 1 and 2, we drop this variable from the rest of the regressions to prevent clutter and potential multicollinearity. Next, we consider the role of education on unbundled democracy while accounting for endogeneity by following Barro and Lee (2010) and using the average education of the parents' generation to instrument for education levels lagged one period. Columns 3 and 4 of Table 6 present these results. Once

Table 6
Determinants of democracy, least squares.

Estimation	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2SLS		2SLS		2SLS		OLS	
Dependent variable	PR	CL	PR	CL	PR	CL	PR	CL
PR, $t-1$	0.116	0.0510	0.123	0.0420	0.129^{+}	0.0490	0.146*	0.0644
	(0.077)	(0.047)	(0.076)	(0.057)	(0.071)	(0.044)	(0.071)	(0.046)
CL, $t-1$	0.319***	0.290***	0.370***	0.333***	0.360***	0.323***	0.346***	0.317***
	(0.084)	(0.052)	(0.094)	(0.063)	(0.081)	(0.052)	(0.079)	(0.051)
Ln GDP pc, $t-1$	0.315	0.0973						
	(0.334)	(0.116)						
Ln Pop, $t-1$	0.0841	0.00221						
	(0.289)	(0.118)						
Ave. Yrs. School, $t-1$			0.159	0.133				
			(0.223)	(0.190)				
Urbanization, $t-1$					−0.00197	0.00185		
					(0.003)	(0.003)		
Oil reserves, $t-3$							−3.974*	−1.970
							(1.584)	(1.475)
Country Dummies	Y	Y	Y	Y	Y	Y	Y	Y
Year Dummies	Y	Y	Y	Y	Y	Y	Y	Y
Observations	757	757	708	708	768	768	786	786
Num countries	131	131	118	118	128	128	131	131
R-squared							0.823	0.849
Instruments used:	Savings rate ($t-2$), LnPop ($t-3$)	Ave Yrs School Parents ($t-3$)	Urbaniz. ($t-3$)	–	–			

Robust standard error clustered by country in parentheses.
*** $p < 0.001$.
** $p < 0.01$.
* $p < 0.05$.
$^{+}$ $p < 0.1$.

again, the earlier results on the persistence and complementarity of civil liberties relative to political rights continue to hold just as before. Education, however, has a statistically insignificant impact on unbundled democracy at any reasonable level of significance. Adding the level of female education instrumented with average education of mothers yields the same results as above (these are not presented for brevity). We present the results of adding urbanization by itself, again using its three period lag as an instrument. Columns 5 and 6 present the results, which are essentially the same as the ones for education. Thus, neither of these aspects of the modernization hypothesis affects our basic results on the persistence and complementarity of civil liberties relative to political rights.

Finally, we try to capture exogenous variation in natural resource wealth through our measurement of oil resources. Oil is one variable that makes the empirical impact of the economic natural resource curse most salient, (Mehlum et al., 2006). Furthermore, as noted in the introduction, it has been shown to generate a political natural resource curse in the case of democratization when measured in terms of endowments. The general literature on democratization, however, has relied on oil exports (either in absolute value or relative to GDP) or dummy variables related to them, e.g., Barro (1999) or Benhabib et al. (2011) and on oil rents, Hegre et al. (2012). Oil rents are defined as (price − cost) * production but the latter quantity in particular can be a source of reverse causality with respect to institutional variables such as democratization.

Use of data on quantity of oil reserves as a proxy for oil rents mitigates endogeneity problems, as changes in these reserves are primarily related to endowment changes through discoveries and can thus be viewed as exogenous. We use oil reserves lagged three five year periods (i.e., 15 years) because the lag between discovery and first production is often two to ten years long (Laherrere, 2003). In addition, these longer lags are useful given the inclusion of lagged values of the two dimensions of democracy in our regressions. Finally, the time fixed effects are likely to account for changes in global oil prices that affect contemporaneous oil rents.

Results are presented in the last two columns of Table 6. They show that the persistence and complementarity effects of civil liberties relative to political rights continue to hold just as before. Interestingly, the political resource curse on democratization through political rights found by Tsui (2011) holds in this setting at the 5% level of significance, but there is no such effect for civil liberties even at the 10% level. Finally, in the appendix (Table A5) we present comparable results using OLS and one period lags for all the variables mentioned above (except for female education and oil reserves). We also present results for two OLS regressions with all the variables, including female education and oil reserves, at the same time in the last two columns. Our two main results on civil liberties relative to political rights also hold in all these OLS settings.

Table 7
Determinants of democracy, SGMM.

Dependent variable	(1) PR	(2) CL	(3) PR	(4) CL	(5) PR	(6) CL	(7) PR	(8) CL
PR, $t-1$	−0.0604	0.0971	0.195	0.0318	0.0779	0.0891	0.175	0.212
	(0.298)	(0.219)	(0.243)	(0.156)	(0.244)	(0.182)	(0.312)	(0.197)
CL, $t-1$	1.014**	0.616**	0.571+	0.509*	0.791*	0.443	0.747*	0.362
	(0.347)	(0.301)	(0.329)	(0.259)	(0.387)	(0.336)	(0.364)	(0.308)
Ln GDP pc, $t-1$	0.0214	0.00948						
	(0.025)	(0.019)						
Ln Pop, $t-1$	0.0179+	−0.00488						
	(0.010)	(0.009)						
Ave. Yrs. School, $t-1$			0.0289	0.0422**				
			(0.026)	(0.016)				
Urbanization, $t-1$					0.00146	0.00266+		
					(0.002)	(0.001)		
Oil reserves, $t-3$							−2.039	−1.592
							(3.831)	(3.579)
Observations	524	524	472	472	512	512	524	524
Num countries	131	131	118	118	128	128	131	131
Num instruments	25	25	24	24	24	24	20	20
Hansen J (p-value)	0.329	0.189	0.0943	0.195	0.207	0.290	0.415	0.636
Diff-in-Hansen (p-value)	0.928	0.921	0.481	0.514	0.585	0.792	0.297	0.370
AR(3) (p-value)	0.296	0.146	0.898	0.199	0.357	0.183	0.893	0.852
Num lags used as instruments ⩾	3	3	3	3	3	3	3	3
Instruments for Endogenous Variables	Savings rate ($t-2$), LnPop ($t-3$)		Ave Yrs School Parents ($t-3$)		Urbaniz. ($t-3$)		–	–

*** $p < 0.001$.
Robust two-step standard errors estimated with Windmeijer (2005) small-sample corrections in parentheses. Year fixed effects included in all specifications; country fixed effects excluded, following Roodman (2009).
** $p < 0.01$.
* $p < 0.05$.
+ $p < 0.1$.

Table 8
Splitting sample by muslim share of population.

Estimation	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS				SGMM			
Subsample: Muslim share	<2.5%	<2.5%	⩾2.5%	⩾2.5%	<2.5%	<2.5%	⩾2.5%	⩾2.5%
Dependent variable	PR	CL	PR	CL	PR	CL	PR	CL
PR, $t-1$	0.201+	−0.00163	0.0770	0.116	0.521*	0.172	0.0267	−0.139
	(0.109)	(0.057)	(0.089)	(0.073)	(0.230)	(0.181)	(0.410)	(0.229)
CL, $t-1$	0.338**	0.367***	0.397***	0.282***	0.445+	0.690**	0.873+	0.777**
	(0.115)	(0.068)	(0.100)	(0.077)	(0.260)	(0.228)	(0.470)	(0.263)
Country dummies	Y	Y	Y	Y	N	N	N	N
Year dummies	Y	Y	Y	Y	Y	Y	Y	Y
Observations	390	390	396	396	260	260	264	264
R-squared	0.856	0.869	0.702	0.733				
Num countries	65	65	66	66	65	65	66	66
Num instruments					23	23	23	23
Hansen J p-value					0.459	0.109	0.133	0.324
Diff-in-Hansen p-value					0.147	0.0961	0.486	0.121
AR(3) p-value					0.203	0.197	0.650	0.284
Lags used as instruments ⩾					3	3	3	3

Robust standard errors clustered by country in parentheses in columns 1–4. Robust two-step standard errors estimated with Windmeijer (2005) small-sample corrections in parentheses in columns 5–8. Country fixed effects excluded in columns 5–8 following Roodman (2009).
*** $p < 0.001$.
** $p < 0.01$.
* $p < 0.05$.
+ $p < 0.1$.

In Table 7, we consider the corresponding results to Table 6 while correcting for dynamic panel bias with SGMM. With respect to persistence and complementarity effects the overall results in the first two rows of Table 6 are very similar to what we found before, especially with respect to the complementarity effect. The latter is substantial and statistically significant,

at least at the 10% level, in all four cases (columns 1, 3, 5 and 7). The complementarity effect for PR (columns 2, 4, 6 and 8) is always statistically insignificant at any reasonable level. The persistence effect for CL is always substantial in size, and statistically significant at the 5% level in two of the four cases (columns 2 and 4) whereas the persistence effect for PR is statistically insignificant at any reasonable level in all four cases (columns 1, 3, 5 and 7).

With respect to covariates, the results are slightly more varied than in Table 6: population is positive for PR and statistically significant at the 10% level (column 1); education is also positive and statistically significant at the 1% level for CL (column 4); and urbanization is also positive and statistically significant at the 10% level for CL (column 6). Tests of over-identifying restrictions and auto correlation are all satisfied at reasonable levels, except perhaps for the Hansen *J*-test in column 3, which is close to but still below 10% for PR in the education regression. While we have the same number of countries in every corresponding column of both tables, Table 7 has far fewer observations than Table 6 in every column. Summing up, the two main empirical implications of our conceptual framework also hold in these settings, namely $\alpha_2 > \alpha_1$ and $\beta_1 > \beta_2$.

Last but not least we turn to the impact of the proportion of Muslims in the population on democratization. We were unable to obtain enough reliable data over time periods on this variable to perform exactly the same exercise as before. Instead we use the most reliable data source that included all of our 131 countries (the Pew Forum on Religion & Public Life[7]) to split the sample into a similar number of countries with a low proportion of Muslims (<2.5%) and a high proportion of Muslims ($\geqslant$2.5%) and re-estimated a similar empirical specification to the ones in Tables 5 and 6 on these split samples. Table 8 presents the results in two parts. The first part shows the OLS results; the second part shows the SGMM results correcting for dynamic panel bias. Both sets of results confirm our earlier findings on the complementarity and persistence effect of civil liberties relative to political rights for both samples. As a check we also re-estimated these specifications for a much smaller sample with a higher cut-off for proportion of Muslims (>50%) and again the complementarity and persistence effects of civil liberties relative to political rights were very similar to earlier findings for both OLS and SGMM. These results are presented in Table A6 of the Statistical Appendix.

7. Concluding remarks

Summing up, unbundling democracy in terms of two dimensions, political rights and civil liberties, provides an encompassing framework for analyzing democracy. At the conceptual level, it contains electoral democracy as construed by Schumpeter and his followers as a special case. At the empirical level, our main contribution in terms of importance provides evidence on civil liberties as a previously overlooked precondition for electoral democracy, and on the lack of symmetry of this complementarity result with respect to political rights. A subsidiary contribution at the empirical level provides evidence on the relative magnitudes of persistence in both dimensions. Incidentally, while civil liberties exhibit quantitatively larger persistence effects than political rights, it does not follow that there are no persistence effects for political rights.

Notably, our empirical results imply that promoting current civil liberties to generate sustained electoral democracy in the future is likely to be more effective than promoting current political rights to achieve this goal. Undoubtedly, this is the most important policy implication of our analysis. While free and fair elections are necessary for democracy, they are far from sufficient. Indeed, our results suggest why they can be inconsistent with sustained electoral democracy: Namely, if they provide cover for the trampling of civil liberties associated with illiberal democracies.

Conceptually, a key factor in differentiating the two dimensions of democracy lies in the ability of civil liberties to provide citizens with satisfaction directly, while political rights only do so indirectly. Empirically, this fundamental difference and other associated differences between civil liberties and political rights generate a setting where these two dimensions of unbundled democracy change in very different ways. First, the persistence effects of civil liberties on subsequent outcomes are substantial and statistically robust to the inclusion of political rights in the analysis. By contrast, the persistence effects of political rights on subsequent outcomes are far less substantial in magnitude and also far less robust statistically to the inclusion of civil liberties in the analysis. Second, the complementarity effects of civil liberties on political rights are substantial and statistically robust, whereas the complementarity effects of political rights on civil liberties are largely non-existent substantively and statistically.

Our empirical results were obtained with the type of cross-country panel data employed to analyze these issues in the political economy and political science literature. Thus, one of the immediate implications of our analysis is to consider the extent to which similar results hold in a variety of other empirical settings. With respect to micro-oriented ones, it makes sense to consider the design of laboratory and/or field experiments aimed at establishing the extent to which different individuals value civil liberties differently from political rights. A variant of this idea would be to design either type of experiment to evaluate the trade-offs that are made between political rights and civil liberties in different types of cultural or political settings.

A similar implication with respect to other types of data would be to search the historical literature in pursuit of events in which segments of the population acquired a particular aspect of civil liberties or of political rights. For instance, these events could be historical moments when the right to own property or the right to vote was acquired. If either historical data

[7] Available at http://www.features.pewforum.org/muslim-population. We use the estimated Muslim population as a proportion of the total population in 1990 (the earliest available year).

or specific surveys containing data on other political rights or civil liberties were available, one could investigate the impact of acquiring these particular aspects on other aspects of civil liberties or political rights. While the implications mentioned thus far would address the extent to which the validity of our results holds in a wider range of settings, they would require a fair amount of effort and ingenuity in their implementation.

Other implications for existing literature, however, provide contexts that are easier to implement by relying on more easily available data. We mention a few examples below. By focusing on the democratization process, we have emphasized the differential role of political rights and civil liberties in the evolution of unbundled democracy. Nonetheless, similar differential roles arise with respect to the considerable literature on many other aspects of democracy. In all these cases, the omission of civil liberties from the empirical analysis generates the possibilities of substantially different results once they are included. For instance, this consideration applies to the research on the duration and stability of democracy that has arisen after the work of Przeworski et al. (2000) and, more generally, to issues of regime stability, e.g., Morrison (2009). It also applies to the literature on the impact of short-run aggregate shocks on democracy mentioned in the introduction. Similarly, writers focusing on democratization in countries undergoing a transition from socialism have also focused on political rights and structural reforms to the neglect of civil liberties, e.g., the case studies analyzed by Haggard and Kaufman (2008).

Last but not least, we note explicitly two complementary links between our approach and an important recent strand of literature, based on North et al. (2009). First, these authors are up-front in their preface emphasizing the presentation of a conceptual framework rather than "...a formal model that generates explicit empirical tests...about social change (p. xii)." We also have a conceptual framework rather than a formal model, albeit a far more limited one, but it generates explicit empirical tests. Furthermore, we have paid a substantial amount of attention to the measurement of political rights and civil liberties and to extracting explicit empirical tests about social change involving political rights and civil liberties. This aspect of our approach provides one complementary link with their work. A second one relates to the door-step conditions for transitions to an open social order in the economic and political realm (Ch. 5). Attaining high levels of civil liberties can be viewed as a mechanism which implements the door-step conditions in their analysis.

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Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.jce.2014.06.001.

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